

notion that the invisible order of things which continuity requires as antecedent to the visible order, is in any sense *material*. They only assume that it must be conditioned. Indeed, the authors of the "Unseen Universe" have expressed this conviction in the preface to the second edition of their work, in italics, and in language that is not only exceedingly clear, but also extremely strong.

But it seems to be taken for granted on all sides that a man of science can only imagine a mechanical unseen. This is really very hard.

The analogy (however inadequate) furnished by Thomson's vortex atoms, and the invisible fluid which they postulate, is too good an illustration of a novel and difficult conception to be disregarded; but it will have to be laid aside at once, if it can be shown to be necessarily productive of such extraordinary misconceptions even in intellects of the highest order.

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, December 25, 1878

### Force and Energy

SINCE a year or two back, when Herbert Spencer started, in the columns of NATURE, a discussion as to the real meaning of the word "force," most careful-thinking students of mechanics have probably come to the conclusion that either the use of the word "force" must be discontinued as a physical scientific term, or that it must be defined in a different manner from that adopted almost universally by those "doctors" whose writings seemed to weigh so heavily on the brain of "poor Publius." They all agree in saying that in its physical application the word "force" means that which produces *i.e.*, the CAUSE of change of momentum. It is needless to give quotations. They are all, except one, curiously explicit. Germans, French, and English agree. "So sehen wir diese Aenderung als Wirkung irgend einer in demselben thätigen Ursache an; diese URSACHE nennen wir Kraft." (Ritter's "Mechanik," p. 36). "On donne, en general, le nom de FORCE à la CAUSE quelconque qui met un corps en mouvement, ou seulement qui tend à le mouvoir." (Poisson: "Traité de Mécanique," Introduction, p. 2.) Although in Tait's "Recent Advances" we find on p. 11 "that we have not yet quite cast off that tendency to so-called metaphysics which has so often blasted," &c., &c.; yet on p. 16 of the same book there is reproduced the fine old crusty Newtonian maxim to which Thomson and Tait and Tait and Steele cling with such fond reverence: "force is any CAUSE which," &c. Clerk Maxwell gives no formal definition of force in his "Electricity and Magnetism." On p. 5 he simply gives its dimensions. On p. 83 of his invaluable "Theory of Heat" he defines, "force is WHATEVER changes or tends to change," &c. This is a very ingenious mode of escaping the difficulty by simply giving no definition at all. We are told what the result of force is, but not what force itself is. We are told that force is "whatever," which is not very clear. Jeames would hardly think that justice was done him if we asserted that the complete definition of him was "whatever opens a door," and made no mention of the fact of his humanity or of his grand plush breeches. It is, in fact, a confusion between a statement of the mode of measuring quantitatively the force, and the definition of the force itself. A physical definition should certainly show clearly what the proper way of measuring the quantity is; but this latter is not the definition itself. Moreover, there may be different almost equally good modes of measurement, all leading to the same numerical result. Clerk Maxwell's definition is clear as to a mode of measuring force, but furnishes absolutely no information as to the nature of the thing intended to be defined. It, therefore, differs from the others in that they are real metaphysical definitions, presumably comprehensible to those who understand metaphysics, while his is no definition at all. Prof. John Perry, in his book on "Steam," adopts the same device as Prof. Clerk Maxwell, substituting the word "anything" for "whatever." Rankine forms a remarkable exception. He says that "force is an action between two bodies either causing or tending to cause change in their relative rest or motion." Here the word "cause" is used in such a sound, practical, common-sense way that no one could take exception to such use of it, even in a physical definition, and probably "action," as here used, might be explained clearly enough for all useful purposes as "a changing relation" or "a change of relation." Rankine, however, does not take the trouble to do this last.

Now clearly a cause is a metaphysical entity, if it is an entity

at all, and from the very nature of the difference between metaphysics and physics, a metaphysical entity cannot possibly be made of any use in physical investigations. If, then, the word force is to be usefully employed in physics, it must be defined as something else than a "cause." When we talk of forces, the physical facts the observation of which we think of, are accelerations of momentum; and in his Glasgow lecture Prof. Tait seems half inclined to use "force" and "acceleration of momentum" as synonymous terms. But an acceleration of momentum is a function of one body only; and every one knows that what is mentioned in Rankine's definition is true, namely, that force is a function of two bodies, and can have neither objective nor any other kind of existence except as a relation between two bodies. Seeing that it is so, I beg to lay before your readers for their favourable consideration the meaning of the word force which I have used for several years past. I wish force to be defined as "time rate of transference of momentum." A transference of anything can only take place between one body and another, and in the transference the amount transferred from the first body to the second is necessarily equal to the amount transferred to the second body from the first. This might seem to be such a truism as to be a mere repetition of words; but we must remember that it is the law of motion which the "transcendently lucid" Newton discovered from his extensive physical experience; and, in order to discountenance scepticism, we might add, by way of parenthesis, that during the transference no spilling takes place.

"Poor Publius" might thus get a hint that there is such a physical fact as conservation of momentum which is independent of all formal definitions. If momentum is conserved, *i.e.*, if it has an enduring existence so that at one time there is no more nor less of it than at another, then during a direct transference of some of it from one part of the system in which it is lodged to another part, the amount lost by the one part must evidently be the same as that gained by the other part. Thus an acceleration or time-rate of gain of momentum to one part necessarily implies a simultaneous equal time-rate of loss of momentum from another part, and also a simultaneous equal rate of transference of momentum from that other to the first part. All these three rates have directions inasmuch as they are time-rates of directed quantities. The first is a rate of gain of momentum, which momentum has a certain direction. If that direction be reckoned positive the gain is one of positive momentum, and the acceleration is naturally reckoned as positive. The second is a rate of loss of momentum of the same direction, *i.e.*, a loss of positive momentum which is equivalent to a gain of negative momentum, and therefore this time-rate is naturally reckoned negative. The meaning of this is simply that the proper physical sign to ascribe to acceleration of momentum is the directional sign of the momentum gained. The two opposite signs of the above two rates have given rise to the idea of two equal and opposite forces acting between the bodies. If the forces were located IN the bodies and not BETWEEN them, the phraseology would be consistent with Tait's definition of force as simply "acceleration of momentum." But I do not hesitate to say that this idea of force is quite unnecessarily out of accord with the commonly received notion of force as a mutual action or relation between two bodies, because in this view force would distinctly have reference to only one body. If, however, we use force to mean the transference of momentum, there is, of course, only one force between the two bodies. The question is what sign is to be given to this force, and it is not quite easy to answer. Force is in this view a flux, a rate of flow of momentum. This flow takes place in a certain direction, and it is the flow of a directed quantity. Are we to take the direction of the flow or the direction of the momentum that flows, to determine the proper sign of the force? These two directions need not be the same. Thus in a bar subjected to tension the flow of momentum is in the direction opposite to that of the momentum itself. In a bar in compression the flow of momentum takes place in the same direction as that of the momentum. In a mass subjected to shearing stress the direction of the flow is perpendicular to that of the momentum. In the case of the attraction of gravitation between two bodies the direction of the flow of momentum is always the exact opposite of that of the momentum that flows from one to the other in whatever way the two may be moving. In the case of impact if we take the direction of the flow of momentum as that of the perpendicular to the surfaces that touch during impact drawn from the body that loses momentum towards the body that gains momentum, then this direction of

flow may make any angle within certain limits with that of the momentum exchanged. If we are to adhere strictly to the ordinary conventions with regard to the directions of forces, it is clear that we would need to consider the transference of momentum which we term force, only with reference to the direction of the momentum transferred, and without any reference to the direction of transference. It is, however, evident that this latter direction is of very great importance in physical investigation, and it is a matter worthy of serious consideration whether or not force should not be considered a two-directional quantity, one into whose definition two directions enter. Impact is a difficult subject, perhaps, just because of the large possible variation of the difference of these two directions. All other forces (rates of transference of momentum), except those involved in impact may be divided into three simple classes corresponding to compression, tension, and shear.

In considering stress, the phrases "transmission of momentum" and "rate of transmission of momentum" are as convenient, perhaps, as the corresponding phrases with "transference" substituted for "transmission."

The most obvious objection to this definition of force is that a force may be applied to a body, and yet it receives no momentum. The objector would probably say that though the force be applied, yet there may be no momentum transferred to the body. But this would be quite wrong, as can be most easily recognised if Prévost's theory of exchanges of heat by radiation and the similar theory for conduction of heat be recalled to mind. A body may quite easily have simultaneously equal amounts of opposite momentum transferred to it. These will balance, and its centre of gravity will suffer no acceleration of velocity. This remark will make it evident that the theory of force gives an easy and unhesitating answer to the much-debated question as to whether there are really such things as unbalanced forces. A transference of momentum between two bodies may just as readily be unbalanced as balanced. Let us consider this balancing of transferences of momentum more particularly. Let a body have momentum transferred to it by the pressure of another body upon a certain portion of its surface. This can be balanced in different ways. It may be balanced by a perpendicular pull applied to a portion of the surface parallel to that to which the pressure is applied, and facing the same way, *i.e.*, on the same side of the body. The directions of the momenta transferred at these two surfaces are the same, but the directions of transference or flow are opposite. Or the pressure may be balanced by an oppositely directed pressure upon a parallel surface facing the other way. In this case the directions of the momenta transferred at the two surfaces are again the same, while the directions of flow are also the same. In all cases when a body is kept in balance by transferences of momentum going on through its different surfaces, it is evident that for any amount of momentum of a given direction transferred into it at one surface an equal amount of momentum of the same direction must be transferred out of it at some other surface. The directions of transference or flow at these two surfaces may be relatively any whatever—they are quite independent. The balance of the body, with respect to the velocity of its centre of inertia, is quite uninfluenced by the directions of the momentum-flows through its different surfaces. But evidently the state of stress and strain throughout the interior of the body depends a great deal upon the relative directions of these flows as well as upon the relative positions of the surfaces.

But, as regards the direction of the momentum, it must be remembered that this depends upon what we arbitrarily choose to be our standard positive direction, whereas the equilibrium of the body acted on certainly does not depend in the least upon that arbitrarily chosen direction. Thus, as in the above example, let the body 2 be kept in equilibrium by the equal and opposite pressures of the bodies 1 and 3 on its opposite faces. The question is whether momentum is being transferred from 1 to 2 and from 2 to 3, the momentum transferred having also this direction; or whether both the flow and the momentum flowing have exactly the opposite direction, *viz.*, from 3 through 2 to 1. If we have a standard positive direction to go by, and if 1 is not in equilibrium, but is being stopped in its motion by impact on 2, then the above question is easily answered at once. But if 1 is in equilibrium as well as 2, we must, in order to answer the question, look beyond 1 to find out the direction of the other transference of momentum, which, along with that between 1 and 2, keeps 1 in equilibrium. If this other trans-

ference takes place between 1 and another body which is again in equilibrium, it would be necessary to go back still another step in order to find out in which direction the flow is really taking place. If the whole system of which these bodies form parts is everywhere in equilibrium, *i.e.*, all the parts at rest relatively to each other, we would in this way travel from one body to another in a complete circuit in search of some point which would disclose the real direction of flow, but without ever coming to any such point. Because, following round the circuit, we would again come back to 3 and 2 and 1. The choice of a standard direction as the positive one does not help us in the least to come to even a formal conclusion. We remain, however, sure of two things—first, that there is really a continual flow of momentum taking place all round this circuit in the system; and, secondly, that the direction of this flow is at some places, which we can definitely specify, in the same direction as the momentum transferred, and at some other places, equally easily specified, in the opposite direction. Take as an example a piano. We may suppose the upper horizontal bar of the frame to which the strings are attached to be continually losing upward momentum, which is being continually received by the top parts of the tightened strings. This upward momentum the strings are continually transmitting downwards from particle to particle, and at the foot of the strings it is delivered to the bottom horizontal bar of the frame. This bottom bar transmits the upward momentum horizontally, each section being in shear, to the vertical sides of the frame. The transmission down the wire is in the direction opposite to that of the momentum transmitted; in the horizontal bottom bar the direction of transmission is perpendicular to that of the momentum, through the sides this momentum flows upwards, that is, in the same direction as the momentum itself, and finally, it is transmitted again horizontally through the upper bar to be redelivered to the strings. This explanation is completely satisfactory in accounting for the conditions of strain of the various parts of the piano. But to explain these conditions an equally satisfactory hypothesis would be that a stream of downward momentum is continually circulating through the piano in the same circuit as the above, but in the opposite direction round that circuit. Or again we might suppose two opposite circulations to be continually going on, one of upward momentum and the other of downward momentum. But whichever of the three hypotheses we may adopt, we always have the flow in the string which is in tension opposite to the momentum flowing through it, and the flow through the horizontal bar perpendicular to the momentum, and the flow through the sides of the frames in the same direction as the momentum transmitted.

Which of the three is to be chosen, or is it of any consequence that we should know which should be taken? The question is not one that can be made to have any degree of unreality in appearance by merely measuring the motions relatively to one thing or another. It is not whether the momentum transferred is upward or downward relatively to the centre of the earth, or relatively to the sun or to the stars. It is, what is the direction of this momentum relatively to the centre of inertia of the piano frame itself, whether this relative momentum is directed from one end of the piano towards the other or from that latter to the former, and the answer to this question is quite independent of what we arbitrarily choose to call the absolute velocity of the centre of inertia of the whole structure.

I will venture to say that the correct answer is that there are two opposite streams of equal amounts through the structure. What is meant by equal amounts is, of course, that the opposite rates of transference through any section are numerically equal. The simplest and clearest proof is this very simple one: If there were only one stream circulating in one direction, since from the above it is clear that the momentum flowing along in this stream is at every point of it of the same direction, and since the stream is a continuous steady one, every part of the structure through which this stream flows would have the velocity corresponding to this momentum, and in consequence the centre of inertia of the structure would have a certain velocity in the same direction. The inconsistency of this result with the datum from which we started, namely, that the momentum transmitted was to be measured relatively to the centre of inertia, need not be pointed out. To look at the question in another way, let us only consider what this momentum, this thing that is being transferred from particle to particle, really is, *viz.*, mass multiplied by velocity, and we cannot fail to come in a moment to the conclusion that these streams are simply streams of molecular vibration.



And since each particle maintains constantly the same average position relatively to the centre of inertia of the whole, it is evident that its alternate opposite displacements and velocities relatively to that centre of inertia must be numerically equal. A certain particle has first a certain velocity in one direction, and immediately afterwards has a numerically equal velocity in the opposite direction. This change cannot take place except by its transferring to the next particle the same numerical amount of momentum of one direction as it receives from that particle of momentum of the opposite direction. In this way constant streams run in the two opposite directions, the momentum flowing along one having the opposite direction to that flowing along the other, and equal numerical amounts of these oppositely directed momenta flowing past any given sections of the two streams per unit of time.

As a sort of parenthesis let me give the following symbolic statement of the foregoing. Let  $V$  be the velocity of flow of either of these two opposite streams, and  $\mu$  the mass per unit volume of the material, and  $v$  the average numerical velocity of the particles. Then since at any given instant half the particles must be supposed to be moving in one direction, and the other half in the opposite direction, the amount of momentum of one direction passing per unit of time through any section of unit area of the correspondingly directed stream, is  $\frac{1}{2} V \mu v$ . A numerically equal amount of oppositely directed momentum is flowing per unit of time through the same unit section in the opposite direction. Observe that the material through which these two streams are flowing is in "balance," "in equilibrium." Suppose the one stream to lead out of, and the other to lead into, an unbalanced mass, which mass suppose not to be losing or gaining momentum except by these two streams. By means of the one it loses, say  $(+\frac{1}{2} V \mu v)$  per unit of time. By means of the other it gains  $(-\frac{1}{2} V \mu v)$  per unit of time. The amount of positive momentum it transfers to the balanced material for unit of time is, therefore,  $+V \mu v$ , and this is the rate of transference of momentum from the unbalanced mass to the balanced material, and through this latter. If the ratio of comparison or extension, *i.e.*, the strain of this balanced material, be called  $e$ , then what we usually call its modulus of elasticity is  $E$  given by the equation  $eE = V \mu v$ . If we insert in this expression the proper value of  $V = \sqrt{\frac{E}{\mu}}$ , the velocity

of transmission of longitudinal vibration, we obtain a value of  $v = e \sqrt{\frac{E}{\mu}}$ , similar to that deduced by De St. Venant for the

first stage of an impact, during which a single *unbalanced* wave of momentum is running forward through the body impinged on. But the important point to notice is that the rate of transference of momentum per unit area is the product of a mass per unit volume ( $\mu$ ) and of two velocities ( $V$  and  $v$ ). In unbalanced transmission these two may be in the same direction, in which case the mass being accelerated is in compression, or they may be in opposite directions, in which case the accelerated mass is in tension; or they may be at right-angles, in which case the accelerated mass is in shear. In balanced transmission if in the one stream the velocity of flow is in the same direction as that of the flowing momentum, then also in the opposite balancing stream these two velocities have the same directions and the material is in compression, the strain being double that which would occur if either of these opposing streams existed by itself unbalanced in the material. Similarly for a balanced state of tension and for one of shear.

Considering these reasonings, does it not seem right to make the direction sign of the force, or rate of transference of momentum, the same as that of the product of these two velocities. The sign of the product of two vectors does not depend on the absolute direction of either, or rather it does not depend on the relation of either to what we arbitrarily choose as our standard direction. It depends only on their mutual relation. Thus we get a definite sign for each force not arbitrary, but real. For a compression force the two vectors have different signs, and their product is a multiple of  $-1$ . For tension, the two being of the same sign, their product is a multiple of  $+1$ . For shear, the two being perpendicular, their product is a multiple of  $\sqrt{-1}$  or of  $-\sqrt{-1}$ . If the direction of transference be oblique to that of the momentum transferred, their product is the sum of a scalar and of a vector. In this case we have compound stress, that is a shear compounded with either compression or tension; and, as every one knows, it is usually convenient to consider the

scalar and the vector parts separately. The question of the *mode* of transmission of momentum corresponding to these main kinds of stress is one of molecular mechanics, into which there is no need of entering here.

ROBERT H. SMITH

(To be continued.)

### Leibnitz's Mathematics

IN perusing some old files of NATURE I came upon the following sentence in a letter from Prof. Tait (vol. v. p. 81) in reference to the invention of the Differential and Integral Calculus:—"Leibnitz was, I fear, simply a *thief* as regards mathematics." Prof. Tait has more than once intimated or expressed a similar opinion.

In reply to this imputation Dr. Ingleby says (NATURE, vol. v. p. 122):—"I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the *last vestige* of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the *unqualified truth* of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, *viz.*, publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives."

Prof. Tait, as far as I know, never responded to the challenge, and I presume there is but one inference to be drawn from his silence.

In a late reading of an account of this controversy from the German standpoint, my interest in the subject has been re-awakened, and I feel a strong desire to see the whole question thoroughly ventilated. Such a consummation must surely be wished by every fair-minded man, and in the name of justice I would ask Dr. Ingleby to be more explicit and do what lies in his power to remove the imputation which has been attempted to be fastened upon Leibnitz.

This question will not down at the bidding of any one, and the documentary evidence alluded to by Dr. Ingleby must sooner or later see the light. Let us have the matter at once and for ever definitely and honourably settled.

A. B. NELSON

Danville, Ky., U.S.A., November 27, 1878

[It is not to be absolutely presumed that, when a busy scientific man lets pass such challenges, he has given up his point. The question has now lost all but a species of antiquarian interest:—still it is worth clearing up. We might begin by asking Mr. Nelson and other defenders of Leibnitz to explain the very singular appropriation which Leibnitz made of "Gregory's Series" *after* having acknowledged whence he got it.—ED.]

### Commercial Crises and Sun-Spots

A SUGGESTION is made by Mr. John Kemp, in NATURE, vol. xix. p. 97, to test the relation of sun-spots to the variation in weight of the cereal grains. Probably the difficulties of giving such a test scientific precision are insurmountable. No doubt these grains do vary in weight from year to year. Of some samples of oats, of crop 1877, contributed by me to the South Kensington Museum, the pound contained 13,642 grains, while the pound of crop 1878 contained 16,870. But there are many varieties of oats, barley, and wheat in general cultivation, each producing grains differing in weight from the others. In an inquiry which I made regarding the weight of the *sterling*, average grains of wheat of crop 1876 from the south of England were found, in an air-dry condition, to weigh as follows: Talavera, 1'01 gr. troy; Chidham white, '76; Sherriff's bearded, '86; Kessingland red, '92; Nursery red, '76; Trump white, '81; Red rivet, 1'00; Lammas red, '89; Hunter's white, '75. And different ears of a given variety of wheat have grains of different weight. If six or eight culms come up on one stool, the largest ears have the heaviest grains. In general, the larger flower-cups in an ear, contain the heavier grains. Then, there is scarcely such a thing to be found as a crop of one pure variety. Any variety rapidly gets mixed with others. And, supposing that a plot were set aside for a pure variety, year after